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The Intergenerational Costs of Crime: Evidence from Maternal Victimization in Brazil

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Abstract

We study the causal effect of maternal criminal victimization on child health using linked police reports and birth records from Brazil. Focusing on robbery and theft - everyday crimes not involving physical injury - we show that victimization during pregnancy increases low birthweight by 6.9 percentage points, with effects particularly pronounced among socioeconomically disadvantaged mothers. These effects are comparable in magnitude to those documented for physical assaults, indicating that stress and economic disruption alone adversely affect fetal development. We also document persistent effects, including elevated hospitalization and ICU admission rates in early childhood, pointing to significant intergenerational costs.

JEL classification

I12, J13, K42, O12

Keywords

victimization, crime, birth outcomes, health investments

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The use of ⓡ signals that the authors' names have been randomized using a uniform distribution.

1 Introduction

Crime is among the most pressing social concerns worldwide, not only because of its immediate physical and psychological toll, but also due to its broader social and economic consequences. In Brazil – where crime rates rank among the highest globally – robberies and thefts are pervasive features of everyday life and frequently involve threats, intimidation, or the risk of violence. Exposure to such events generates acute stress and fear among victims and, according to surveys such as Latinobarmetro, consistently ranks as one of the most salient public concerns in the region.[1] Beyond the direct financial losses from stolen property (Cohen et al., 2004), criminal victimization has been linked to a wide range of adverse outcomes, including injuries (Miller et al., 1993), reductions in life expectancy from violent crime (Auger et al., 2016), lost productivity (Cabral et al., 2016), and substantial psychological costs (Hanson et al., 2010). Whether these consequences extend to the next generation through effects on children exposed to crime in utero is the question we address in this paper. We study these issues in the state of Minas Gerais, Brazil's second most populous state, where crime rates are high and unusually rich administrative data allow us to link police reports to vital statistics.

A growing literature shows that exposure to stress and violence during pregnancy adversely affects birth outcomes. Earlier work focused on rare or extreme events terrorist attacks (Camacho, 2008; Quintana-Domeque and Ródenas-Serrano, 2017), the 9/11 attacks (Ecclestone, 2012; Eskenazi et al., 2007), and armed conflict (Mansour and Rees, 2012) while more recent studies examine crime exposure directly. Foureaux Koppensteiner and Manacorda (2016) find that municipal-level exposure to homicides in Brazil reduces birthweight, and Brown (2018) document that Mexico's surge in drug-war homicides altered birth outcomes through changes in prenatal care utilization. A common feature of these studies is that exposure is measured at the geographic level, which introduces uncertainty about whether individual pregnant women were directly affected and tends to attenuate estimated effects. Closest to our work is Currie et al. (2022), who link individual-level records of physical assaults against pregnant women in New York City to birth outcomes and find large effects driven by direct injury. We focus instead on non-violent property crimes and trace effects beyond birth into early childhood.

We do so by linking the universe of police reports including detailed information on individual

[1]Latinobarmetro provides regular public opinion surveys for countries in Latin America (http://www.latinobarometro.org). In survey responses, crime and violence regularly score highly when participants are asked about the country's most important problems.

victims to the universe of birth records, allowing us to study the effects of direct, individual-level criminal victimization during pregnancy. This design substantially reduces measurement error in exposure and allows us to recover the magnitude of impacts that may be obscured in aggregate analyses. We focus on theft and robbery the most prevalent forms of everyday victimization, accounting for 63 percent of all offenses with a female victim[2] for two reasons that are central to identification. First, these offenses are overwhelmingly committed by strangers, making victimization plausibly exogenous to the characteristics of the victim and her pregnancy. We reinforce this by excluding all cases in which the perpetrator is known to or related to the victim, thereby abstracting from domestic or relational violence.[3] Second, these crimes do not typically involve physical injury, allowing us to isolate consequences that operate through psychological stress, disruption of daily routines, and economic loss rather than direct bodily harm.

Our analysis yields several key findings. Maternal victimization during pregnancy reduces average birthweight by approximately 3 percent of a standard deviation and increases the probability of low birthweight by 6.9 percentage points – an 85 percent increase relative to the baseline – and has even larger relative effects on very low and extremely low birthweight. These effects are particularly pronounced among socioeconomically disadvantaged and non-married mothers. Beyond birth outcomes, victimization increases hospitalizations in the first three years of life by 0.7 percentage points (a 6 percent increase). We also find modest increases in very short gestation and significant reductions in prenatal care utilization, suggesting behavioral responses that may amplify adverse fetal health effects. Strikingly, these effects are of comparable magnitude to those found by Currie et al. (2022) for physical assaults during pregnancy, suggesting that the stress and economic disruption from everyday property crime alone - without direct physical harm - is a powerful driver of adverse birth outcomes.

Our findings reveal that everyday criminal victimization imposes substantial and lasting costs on the next generation. By measuring exposure at the individual level rather than through geographic aggregates (Quintana-Domeque and Ródenas-Serrano, 2017; Mansour and Rees, 2012; Foureaux Koppensteiner and Manacorda, 2016; Brown, 2018), we show that the true magnitude of these intergenerational effects is considerably larger than previously understood. By focusing on non-violent property crime and isolating the stress and disruption channel from direct physical harm we demonstrate that even the most common forms of victimization, not only rare or extreme events,

[2] The remainder is distributed across 501 distinct crime categories.

[3] For our sample of pregnant women, we exclude 1,331 cases with a documented relationship between victim and perpetrator.

carry consequences for fetal development. And by tracing these consequences beyond birth into early childhood, we show that the damage is not transitory: children of victimized mothers face elevated hospitalization rates and more intensive medical care in the years that follow. Our results point to a previously undocumented dimension of the societal burden of crime one that extends across generations with important implications for both crime-reduction policy and the design of support systems for pregnant women.

The remainder of the paper is organized as follows. Section 2 describes the datasets used in the analysis. Section 3 outlines the identification strategy. Section 4 presents the results, and Section 5 concludes.

2 Data

To estimate the causal effect of criminal victimization on birth outcomes, we construct a novel dataset by linking four comprehensive administrative data sources from Brazil: (i) the universe of birth records, (ii) hospital admissions, (iii) death records, and (iv) police crime reports. The coverage and detail of these datasets allow us to measure exposure to crime at the individual level and follow children from birth through the first year of life and, for hospitalizations, up to three years. The analysis focuses on Minas Gerais, Brazil's second most populous state, which is broadly representative of large Brazilian states in terms of socioeconomic indicators, health system coverage, and crime rates, while also providing unusually comprehensive administrative records that enable individual-level linkage across data sources.[4].

2.1 Birth data

We use data from the Brazilian Ministry of Health's Live Birth Information System (SINASC)[5] for the period 20112017. These birth certificates cover the universe of live births in the state of Minas Gerais and contain detailed information on maternal characteristics, pregnancy history, delivery, and newborn health outcomes.[6]

We calculate gestational length using the mothers reported date of last menstrual period and the

[4]Minas Gerais is Brazils second most populous state, home to about 11% of the national population (21 million in 2017). Its Human Development Index (0.731 in 2010) is close to the national average, and its crime rates, while high, are comparable to those observed in several other large states such as Rio de Janeiro or Bahia (United Nations Development Programme et al., 2013)

[5]*Sistema de Informações sobre Nascidos Vivos*, in Portuguese.

[6]Where births occur in hospitals – roughly 99 percent of cases – records are transmitted directly to the state secretariat of health. For home births, midwives report information directly, ensuring near-universal coverage.

date of delivery. We construct indicators for preterm (<37 weeks) and very preterm (<32 weeks) births. The SINASC data also include information on prenatal care utilization, which is free of charge in the public health system and of relatively high quality in Brazil (Victora et al., 2011). On average, mothers report just over eight prenatal visits.[7] Over our sample period, 1,373,656 births occurred in Minas Gerais. This large sample provides ample statistical power to detect effects even though victimization during pregnancy is relatively rare, and enables the analysis of heterogeneous effects across maternal subgroups. As mothers with longer gestational periods are more likely to be victimized during pregnancy, we assign a standardized 280-day gestation window to all mothers regardless of observed gestational length, using the date of the mother's last menstrual period as the starting point. We discuss the identification rationale for this choice in Section 3.

2.2 Hospital admissions data

We link birth records to hospital admissions data from the Brazilian Hospital Information System (SIH)[8], which covers the universe of admissions to hospitals in the public health system (SUS).[9] SIH provides information on the timing and duration of admissions, costs to SUS, the hospital department used (including ICU and neonatal ICU), and detailed diagnoses coded according to WHO ICD-10 classifications.

2.3 Child mortality data

We measure child mortality by linking SINASC birth records to the Mortality Information System (SIM)[10], which covers all natural and non-natural deaths in Brazil. SIM provides information on cause of death and characteristics of the deceased. We construct standard indicators of early neonatal (within 1 week), neonatal (within 4 weeks), and infant (within 1 year) mortality.

2.4 Victimization data

Finally, we use the Register of Occurrence (SRO)[11], which contains the universe of crime reports filed with the police between 2011 and 2017. A new record is created whenever police are called to

[7] Visits include extensive screening for risk factors such as diabetes, pre-eclampsia, infections, and ultrasound scans.

[8] *Sistema de Informações Hospitalares*, in Portuguese.

[9] This differs from SINASC, which includes all births, including those in private hospitals. As a result, our hospitalization outcomes may underestimate the true effects if victimized mothers disproportionately use private hospitals.

[10] *Sistema de Informações sobre Mortalidade*, in Portuguese.

[11] *Sistema de Registro de Ocorrência*, in Portuguese.

an incident or when a victim (or third party) reports a crime.

Our analysis focuses on theft and robbery, which offer three advantages for our setting. First, these offenses are overwhelmingly committed by strangers, making victimization plausibly exogenous to the characteristics of the victim and her pregnancy. Second, they typically do not involve physical injury, allowing us to isolate stress-related mechanisms from direct bodily harm. Third, they are by far the most prevalent crimes involving individual victims together accounting for approximately 63 percent of all reported crimes with a female victim which provides the statistical power needed to detect effects on rare outcomes. The remaining crimes are spread across 501 distinct categories, most of which occur infrequently and offer limited statistical power.[12]

Using detailed information in the SRO on the relationship between victims and perpetrators, we exclude all cases in which the victim and perpetrator are known to each other or directly related (1,331 observations). This restriction is central to our identification strategy: unlike violent crimes, which frequently involve acquaintances or intimate partners and where the visibility of pregnancy may itself alter exposure risk or reporting behavior, thefts and robberies by unknown perpetrators are plausibly exogenous to victim characteristics. Our final sample therefore consists of thefts and robberies committed by strangers, which strengthens identification by abstracting from domestic or relational violence. We also deliberately exclude assaults and incidents involving physical violence or injury.[13] While violent victimization during pregnancy is an important topic in its own right, it raises distinct conceptual challenges: physical violence may directly harm the fetus, making it difficult to disentangle stress-related mechanisms from mechanical injury, and the severity of physical harm is often difficult to measure consistently. By focusing on theft and robbery without injury, we isolate the impact of victimization that operates primarily through psychological stress, disruption of daily routines, and economic shock, rather than through direct physical harm to the mother or fetus.

2.5 Data linkages and sample definition

We link birth records to crime reports at the individual level using identifiers including the mother's name, date of birth, and place of residence. Infant mortality records are linked to births

[12]The next largest categories are *threat*, *fraud*, and *physical violence/aggression*, accounting for 8.4, 3.4, and 3.3 percent of crimes, respectively. We exclude crimes classified as *threats* because they are highly heterogeneous, frequently involve known perpetrators, and are subject to substantial reporting and classification ambiguity, which complicates identification and interpretation. Similarly, we exclude the small number of fraud cases.

[13]Our focus on non-assault crimes is complementary to Currie et al. (2022), who study physical assaults during pregnancy; together, the two papers span both direct injury and non-physical stress channels of criminal victimization.

using the unique birth ID when deaths occur within the first year of life. For deaths occurring after the first year, we link mortality records using maternal name, child's date of birth, and municipality of residence. Hospitalization records are linked to birth records using postcode of residence and child's date of birth probabilistically. Unlike the other datasets, which can be matched using unique identifiers, this procedure introduces some potential for linkage error or duplication, and may lower match rates, possibly leading to noisier estimates. Between 2011 and 2017, 1,234 mothers were recorded as victims of robbery and 2,948 as victims of theft during pregnancy. As discussed above, this excludes cases in which the victim and perpetrator are known to each other or directly related, restricting the sample to victimization by strangers. A small number of the remaining cases report the victim as having been injured during the incident; we provide a robustness exercise in the appendix excluding these cases.

To define treatment, we focus on mothers who are victimized in crime once during pregnancy and remove a small number of observations of mothers (225) who are victimized more than once. In the appendix, we provide some robustness to the inclusion of these cases of multiple victimization during pregnancy; their inclusion does not change the estimated coefficients meaningfully.

3 Identification Strategy

Estimating the causal effect of criminal victimization during pregnancy on birth outcomes is challenging because victimization risk may be correlated with unobserved maternal or neighborhood characteristics that also affect fetal health. For example, women living in high-crime areas may differ systematically in socioeconomic status, baseline health, or access to health services, all of which are known to influence birth outcomes.[14] In addition, crime reporting may vary systematically across neighborhoods or individuals, potentially introducing further bias.[15]

Our identification strategy exploits variation in the timing of individual-level victimization during pregnancy, conditional on rich controls and fixed effects. By linking the universe of crime reports to the universe of birth records in Minas Gerais, we observe whether and when a pregnant woman is directly victimized.

[14] A large literature links poverty and poor nutrition during gestation to adverse outcomes beyond birth (Chen and Zhou, 2007; Lindeboom et al., 2010), as well as to adverse disease environments (Rocha and Soares, 2015; Maccini and Yang, 2009; Almond et al., 2011, 2012; Amarante et al., 2016; Bozzoli and Quintana-Domeque, 2016). Socioeconomic disadvantage is also associated with stress, poor health, and short-sighted decision making, all of which can reduce newborn health independent of victimization (Dohrenwend, 1973; Case et al., 2002; Deaton, 2002; Haushofer and Ernest, 2014).

[15] Similar concerns arise in victimization surveys, where self-reporting differs across socioeconomic groups.

After conditioning on neighborhood fixed effects and month-of-conception fixed effects, and focusing on crimes committed by unknown perpetrators and excluding assaults and other violent crimes, the timing of victimization during pregnancy is not systematically related to unobserved determinants of birth outcomes. To this end, we estimate specifications that condition flexibly on location, time, and maternal characteristics.

All specifications include neighborhood fixed effects, which absorb time-invariant local characteristics such as baseline crime levels, socioeconomic conditions, and health infrastructure, as well as month-of-conception fixed effects, which flexibly control for seasonality and aggregate time trends in both crime and birth outcomes. We further condition on a comprehensive set of maternal and pregnancy characteristics, including age, race, marital status, education, occupation, reproductive history (parity, previous stillbirths, birth intervals), and receipt of government transfers.

A key mechanical concern in studies of in utero exposure is that pregnancies with longer gestational length are mechanically more likely to be exposed to events occurring late in pregnancy. To address this issue, we do not infer exposure windows from realized gestation length. Instead, we construct conception dates using the mother's reported date of last menstrual period and assign a standardized 280-day gestation to all pregnancies, regardless of observed gestational length. This approach avoids spurious correlations between exposure and outcomes that may arise if victimization itself affects gestation length (Quintana-Domeque and Ródenas-Serrano, 2017).

Our baseline estimating equation is:

$$y_{int} = \beta_0 + \beta_1 Victim_i + X_i\beta_2 + d_n + d_t + u_{int}, \quad (1)$$

where y_{int} denotes the outcome for mother i in neighborhood n with conception date t, $Victim_i$ is an indicator for whether mother i was victimized during pregnancy, X_i is a vector of maternal and pregnancy controls, d_n and d_t are neighborhood and month-of-conception fixed effects, and u_{int} is the error term. Standard errors are clustered at the neighborhood level.

For β_1 to have a causal interpretation, we require that conditional on observables and fixed effects, the timing of victimization during pregnancy is as good as random. We provide direct evidence on the plausibility of this assumption by comparing observable characteristics of victimized and non-victimized mothers. Appendix Table A2 reports balance tests for the main sample.[16] Differences in maternal age, education, marital status, and reproductive history are modest in

[16] We report standardized mean differences rather than t-tests, since large samples render trivial gaps statistically significant.

magnitude and are addressed by the rich set of controls included in all specifications.

By focusing on theft and robbery committed by unknown perpetrators - crimes with clear legal definitions and relatively uniform reporting incentives - our design already limits concerns about differential reporting ex ante. We nevertheless further assess robustness in three ways: First, we include neighborhood-specific time trends and increasingly granular spatial fixed effects - including postcode and hospital-of-birth fixed effects - to absorb localized crime dynamics and unobserved heterogeneity in health care provision. Second, we restrict the comparison group to mothers who are eventually victimized, comparing those exposed during pregnancy to those exposed after delivery. This timing-based design relies on a weaker identifying assumption and yields similar estimates; Appendix Table A3 shows that observable characteristics are even more closely aligned in this sample. Crucially, this design also directly addresses concerns about reporting selection: since both treated and control mothers filed a police report, any systematic differences between women who report crimes and those who do not are eliminated by construction. Third, we estimate maternal fixed effects models that exploit within-mother variation across multiple pregnancies, thereby eliminating all time-invariant maternal heterogeneity, including persistent health, preferences, and reporting behavior. Appendix Table A4 summarizes observables for this sample. Because identification relies on within-mother variation, remaining differences largely reflect time-varying characteristics such as age progression, parity, and birth spacing, which we explicitly control for in all specifications.

As we show in Section 4, the estimated effects remain stable in magnitude across all approaches, supporting a causal interpretation of the impact of non-assault victimization during pregnancy on birth outcomes. Precision declines in smaller samples, particularly in the maternal fixed effects design, but point estimates are consistently of similar sign and magnitude. While we cannot directly observe unreported victimization, the robustness of the results to alternative samples and maternal fixed effects models mitigates concerns that differential reporting or selection into reporting drives the estimated effects.

4 Results

In this section, we present the results of the effect of criminal victimization during pregnancy on a number of outcomes. Unless otherwise noted, all results are based on our preferred specification with neighborhood and month-of-conception fixed effects and the full set of maternal and pregnancy controls. We begin with birthweight outcomes in Subsection 4.1, then examine heterogeneous effects in Section 4.2. We next turn to additional birth outcomes in Subsection 4.3, before examining hospitalization outcomes in Subsection 4.4.

4.1 Effect of crime victimization on birth outcomes

We first examine the effect of maternal victimization during pregnancy on birthweight (BW) measured in grams, and then study how these effects vary along the birthweight distribution. Table 1 reports the estimates.

Column (2) presents our preferred specification and shows that victimization during pregnancy reduces average birthweight by approximately 13 grams. This estimate is statistically significant and economically meaningful, corresponding to roughly 3 percent of a standard deviation. Estimates are stable across a wide range of alternative specifications, including neighborhood-specific time trends, more granular spatial fixed effects, comparisons among mothers who are eventually victimized, and maternal fixed effects models, as shown in the remaining columns of Table 1.

Estimates are stable across a wide range of alternative specifications, including neighborhood-specific time trends, more granular spatial fixed effects, comparisons among mothers who are eventually victimized, and maternal fixed effects models, as shown in the remaining columns of Table 1. The maternal fixed effects estimate is larger in magnitude (24.9 grams), though not statistically distinguishable from the baseline.[17] The apparent doubling of the point estimate is within sampling variation given the much smaller and less precisely estimated FE sample. Moreover, the maternal FE sample is compositionally different from the full sample: it requires mothers with multiple births during the 2011-2017 window, which selects toward younger and more socioeconomically disadvantaged women.[18] Finally, the direction of the change is itself informative about selection. If unobserved time-invariant maternal characteristics were driving the baseline results - for example, if women who report crimes to the police are inherently more likely to have poor birth outcomes - then eliminating all such heterogeneity through maternal fixed effects should attenuate the estimate toward zero. The fact that the point estimate increases rather than shrinks suggests that, if anything, the cross-sectional estimates understate the true effect of victimization. The stability of the estimates across all these specifications supports the robustness of the baseline result.[19]

[17] The 95 percent confidence intervals of the two estimates overlap substantially: the FE estimate ranges from roughly 51 to +1.4 grams, comfortably encompassing the baseline point estimate of 13.0 grams, so we cannot reject that the two are equal.

[18] We show later in Section 4.2 that for precisely this subgroup, we document larger effect. A larger point estimate in this sample is therefore not only unsurprising, but predicted by our own heterogeneity results.

[19] We provide additional robustness exercises in Appendix Table A1 where we present results excluding the small number ($n = 65$) of victims reported to have sustained an injury during the robbery/ theft, results are largely

The most striking results emerge when examining the lower tail of the birthweight distribution. Using our preferred specification, we find that victimization during pregnancy increases the probability of low birthweight by 6.9 percentage points. Relative to a baseline incidence of 8.0 percent, this represents an increase of more than 85 percent, or roughly 69 additional low-birthweight cases per 1,000 births. Given the well-documented long-run consequences of low birthweight for health, education, and labor market outcomes (Almond et al., 2005; Black et al., 2007; Figlio et al., 2014), this effect is substantial.

Even further down the birthweight distribution, victimization increases the probability of very low birthweight by about 2.5 percentage points relative to a baseline incidence of 11.0 percent, corresponding to a 23 percent increase in risk. For extremely low birthweight, the estimated effect is 0.21 percentage points against a mean of 0.45 percent, implying an increase in risk for extremely low birthweight of nearly 47 percent compared to the baseline. Importantly, the relative magnitude of the effects increases as we move from low to very low to extremely low birthweight, indicating that maternal victimization disproportionately affects already fragile pregnancies. Estimates for these outcomes are robust across alternative specifications, although precision declines in smaller samples such as the maternal fixed effects design.[20]

The magnitude of the estimated effect is larger than those typically reported in studies that rely on indirect or aggregate measures of crime exposure. Much of the existing literature assigns exposure at the level of geographic units – such as municipalities or regions – thereby averaging over many pregnant women who are not directly affected by crime, which naturally attenuates estimated effects. By contrast, our design uses individual-level victimization records, allowing us to capture the full impact of direct exposure.

Our estimates compare closely to Currie et al. (2022), who study physical assaults in New York City - a considerably more severe form of victimization. They find effects on very low birthweight of 1.5 percentage points (61% relative to the mean), about twice as large as ours, although a direct comparison is not possible for most other outcomes.[21] That non-violent property crimes produce effects of broadly comparable magnitude to physical assaults is itself informative about mechanisms. It suggests that the stress and disruption channel alone - without direct physical harm to the mother

unaffected. In Table A2, we include observations with multiple victimization during pregnancy. Again, the results are very similar when compared to our main sample.

[20] Appendix Figure A1 presents estimates separately for theft and robbery. While coefficients are not statistically different across crime types, they are generally larger for theft, except for the low birthweight outcome.

[21] The main outcomes in Currie et al. (2022) use a variety of health at birth indices. The effects they estimate on birthweight and low birthweight are also larger (44.6 grams, 0.008 pp., respectively), but statistically insignificant.

- is a powerful driver of adverse birth outcomes. Moreover, property crimes involve a direct economic shock: the loss of assets or income. In a setting like Brazil, where household incomes are low and savings buffers thin, even a single theft or robbery can represent a substantial disruption, consistent with our finding that effects are concentrated among socioeconomically disadvantaged mothers.

The distributional results along the birth weight distribution show that the consequences of maternal victimization during pregnancy are concentrated in the lower tail of the birthweight distribution. This pattern is consistent with victimization exacerbating fetal vulnerability rather than uniformly shifting the distribution, and helps reconcile our relatively large estimates with the smaller intent-to-treat effects typically found in studies using indirect measures of exposure.

4.2 Heterogeneous Effects on BW

In this section, we examine heterogeneous effects of victimization on birth outcomes across key maternal characteristics (Figure 1). We split the sample by socioeconomic status, maternal education, and marital status, using information from the official birth register. Across specifications, the reductions in birthweight and the increases in the incidence of low, very low, and extremely low birthweight tend to be larger for mothers of lower socioeconomic status, although the differences are not statistically significant. A similar pattern emerges when stratifying by education and marital status. In particular, the adverse effects are accentuated for non-married mothers, who experience larger reductions in birthweight and higher risks of low birthweight compared to their married counterparts.

These results suggest that the consequences of victimization during pregnancy fall disproportionately on mothers who are already disadvantaged. Although not all subgroup differences are statistically significant, the consistent pattern across socioeconomic, educational, and marital dimensions is consistent with a vulnerability-amplification mechanism, whereby women with fewer economic, social, or familial resources are less able to buffer the shocks associated with victimization.

4.3 Effect of crime victimization on additional outcomes

Turning to additional outcomes (Table 2), we find little evidence that victimization meaningfully alters overall gestational length or acute indicators of neonatal distress, but it does appear to increase the risk of more extreme vulnerability and to change maternal health behavior. Victimization has a small and statistically insignificant effect on average days of gestation, yet it significantly raises the probability of very short gestation (less than 224 days) by 0.27 percentage points on a base of 1.41 percent, a 19 percent increase in risk. This pattern is consistent with our main results: victimization does not systematically shift gestation length for most pregnancies, but it pushes a subset of already fragile pregnancies into extreme prematurity. In terms of maternal behavior, we detect a statistically significant reduction in the number of prenatal visits, albeit modest in magnitude relative to the mean. This suggests that victimization during pregnancy induces behavioral responses such as avoidance of clinics, disruptions to routine, or tighter time and budget constraints that may exacerbate adverse effects on fetal growth, particularly later in gestation. By contrast, we find no significant effects on 1- and 5-minute Apgar scores, emergency C-sections, the sex ratio at birth, or early neonatal, neonatal, and infant mortality. Taken together, these results indicate that the main consequences of victimization operate through intrauterine growth restriction, increased risk of extreme prematurity, and reduced prenatal care, rather than through acute perinatal complications or changes in survival at birth.

Beyond these outcomes, we examine several standard indicators of neonatal health and survival. Consistent with much of the literature, we find no statistically significant effects of victimization on 1- and 5-minute Apgar scores, emergency C-sections, or the sex ratio at birth. Likewise, when we link births to deaths in the first year of life, we do not detect any effects on early neonatal, neonatal, or infant mortality. These null results suggest that the main consequences of victimization operate through intrauterine growth and subsequent morbidity rather than through acute perinatal distress or changes in survival probabilities at birth, which is in line with the concentration of our effects in the lower tail of the birthweight distribution and the elevated risk of hospitalization and ICU use in early childhood.

4.4 Effect of crime victimization on hospitalization outcomes

Finally, we examine whether the consequences of victimization extend beyond health at birth to child health outcomes in the first three years of life. For this purpose, we link birth records to hospitalization records. Table 3 presents the estimates using our preferred specification. We find that victimization during pregnancy significantly increases the likelihood of hospitalization in the first three years after birth: the probability rises by 0.7 percentage points, corresponding to a 6 percent increase relative to the mean. Most of this effect is concentrated in the months immediately following birth, as shown in columns (2)(4), although these estimates are somewhat imprecise due to limited statistical power. Consistent with the increased incidence of hospitalization, we also observe higher associated medical costs (measured in logs).

Further, we exploit information on the type of hospitalization recorded in the administrative data. Columns (6) and (7) of Table 3 show that victimization during pregnancy substantially raises the risk of both neonatal and regular intensive care admissions. These estimates are again based on our preferred specification. Specifically, neonatal ICU admissions increase by 0.45 percentage points relative to a mean of 1.88 percent – an effect of nearly 24 percent, or about 45 additional cases per 1,000 births – while regular ICU admissions increase by 0.40 percentage points relative to a mean of 2.23 percent, corresponding to an 18 percent increase, or about 4 additional cases per 1,000 births. These patterns point to more severe and costly health complications. Importantly, they align closely with our earlier birthweight results: the concentration of effects in the lower tail of the birthweight distribution suggests that vulnerable infants are more likely to require intensive medical care, a pattern borne out in the hospitalization data. Moreover, the diagnostic information in Figure A2 indicates that the increase in hospitalizations is largely driven by infectious diseases – possibly reflecting lower immunity among children exposed to maternal victimization in utero – and conditions originating in the perinatal period. This pattern is highly consistent with pregnancy being adversely affected by victimization, reinforcing the interpretation that the effects we uncover operate through compromised fetal environment during pregnancy.

5 Final Remarks

This paper provides novel evidence on the causal effect of direct victimization in crime, specifically robbery and theft, on birth outcomes. We draw on a unique dataset from Brazil that links the universe of police reports to the universe of birth records over the period 2011 – 2017, enabling us to identify the effects of individual-level exposure to crime during pregnancy on fetal and early-life health.

We find that maternal victimization significantly reduces birthweight and worsens other indicators of neonatal health. In our preferred specification, victimization during pregnancy reduces average birthweight by about 13 grams – approximately 3 percent of a standard deviation – and increases the probability of low birthweight by 6.9 percentage points, or an 85 percent increase relative to the mean. The effects grow in magnitude at the lower tail of the birthweight distribution, with especially large impacts on very low and extremely low birthweight. We also find that the adverse effects are more pronounced for socioeconomically disadvantaged mothers, pointing to vulnerability amplification among groups with fewer resources.

The consequences of victimization are not limited to birth outcomes. We document substantial downstream effects on child health, linking birth records to hospitalization data for the first three years of life. Victimization increases the likelihood of hospitalization by about 6 percent, with particularly pronounced increases in neonatal and regular ICU admissions - roughly 4-5 additional cases per 1,000 births in each case. Detailed information on cause of hospitalization suggests that these increases are largely driven by infectious diseases and conditions originating in the perinatal period, highly consistent with our interpretation that maternal victimization compromises fetal development and leaves infants more vulnerable after birth.

We also investigate a broader set of outcomes. Victimization modestly increases the probability of very short gestation and significantly reduces prenatal care utilization, pointing to behavioral responses that may exacerbate adverse birth outcomes. While our data cannot pin down the precise mechanisms, reduced prenatal visits could reflect avoidance strategies, disruptions in routine, resource constraints, or psychological withdrawal. Importantly, this finding highlights an additional channel through which victimization can worsen pregnancy outcomes. For other outcomes – including Apgar scores, emergency C-sections, sex ratio at birth, and infant mortality – we do not find significant effects.

Our results contribute to several strands of the literature. A growing number of studies examine the impact of crime and violence on birth outcomes, often focusing on large-scale shocks such as terrorism, war, or aggregate homicide rates (Quintana-Domeque and Ródenas-Serrano, 2017; Mansour and Rees, 2012; Foureaux Koppensteiner and Manacorda, 2016; Brown, 2018). By leveraging individual-level victimization records, we move beyond intent-to-treat designs that assign exposure at the geographic level and show that direct victimization has distinctly negative effects on fetal health. Our use of rich administrative data further allows us to shed light on mechanisms, including both intrauterine growth restriction and behavioral responses such as reduced prenatal care.

Finally, our findings contribute to the broader literature on the societal costs of crime. While prior work has emphasized the physical and psychological harms of victimization (e.g., Anderson, 1999; Dolan et al., 2005; Brewster, 2014), our results reveal an important and previously undocumented channel: the intergenerational consequences of crime through adverse effects on birth outcomes and early-childhood health. By showing that the costs of crime extend beyond immediate victims to the next generation, our study underscores the broader societal burden of criminal victimization. From a policy perspective, our findings suggest that efforts to reduce crime, alongside programs aimed at safeguarding pregnant women and ensuring continuity of prenatal care after victimization, may yield substantial intergenerational health benefits.

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Table 1: Effect of crime victimization on birthweight

	BW								*Low BW*							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Victim	−10.0673	−13.0352	−12.9146	−11.8169	−10.6388	−13.5040	−10.2779	−24.9425	0.0053	0.0069	0.0070	0.0064	0.0066	0.0078	0.0053	0.0119
(Pregnancy)	(4.6344)**	(4.6011)***	(4.6809)***	(4.4761)***	(4.7726)**	(5.7234)**	(5.0615)**	(13.2175)*	(0.0026)**	(0.0026)***	(0.0027)***	(0.0028)**	(0.0024)***	(0.0031)**	(0.0029)*	(0.0078)
Mean dep. var.	3,159.0410	3,159.0410	3,159.0410	3,159.0410	3,159.0410	3,159.0410	3,150.6929	3,172.0742	0.0804	0.0804	0.0804	0.0804	0.0804	0.0804	0.0830	0.0742
Clusters	32,405	32,405	32,405	899	44,237	192,897	9,570	109,980	32,405	32,405	32,405	899	44,237	192,897	9,570	109,980
Observations	1,433,140	1,433,140	1,433,140	1,432,743	1,422,023	1,056,977	123,114	225,353	1,433,140	1,433,140	1,433,140	1,432,743	1,422,023	1,056,977	123,114	225,353
	Very low BW								*Extremely low BW*							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Victim	0.0024	0.0025	0.0026	0.0023	0.0022	0.0026	0.0021	0.0059	0.0021	0.0021	0.0020	0.0020	0.0018	0.0023	0.0021	0.0037
(Pregnancy)	(0.0011)**	(0.0011)**	(0.0011)**	(0.0009)**	(0.0011)**	(0.0013)**	(0.0012)*	(0.0035)*	(0.0007)***	(0.0007)***	(0.0008)***	(0.0006)***	(0.0007)**	(0.0010)**	(0.0008)***	(0.0028)
Mean dep. var.	0.0110	0.0110	0.0110	0.0110	0.0110	0.0110	0.0121	0.0101	0.0045	0.0045	0.0045	0.0045	0.0045	0.0045	0.0051	0.0045
Clusters	32,405	32,405	32,405	899	44,237	192,897	9,570	109,980	32,405	32,405	32,405	899	44,237	192,897	9,570	109,980
Observations	1,433,140	1,433,140	1,433,140	1,432,743	1,422,023	1,056,977	123,114	225,353	1,433,140	1,433,140	1,433,140	1,432,743	1,422,023	1,056,977	123,114	225,353
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Neighbourhood FE	Yes	Yes	Yes	Yes	No	No	Yes	Yes	Yes	Yes	Yes	Yes	No	No	Yes	Yes
Neighb x Time FE	No	No	Yes	No	No	No	No	No	No	No	Yes	No	No	No	No	No
Hospital FE	No	No	No	Yes	No	No	No	No	No	No	No	Yes	No	No	No	No
Postcode FE	No	No	No	No	Yes	No	No	No	No	No	No	No	Yes	No	No	No
Street FE	No	No	No	No	No	Yes	No	No	No	No	No	No	No	Yes	No	No
Alternative Sample	No	No	No	No	No	No	Yes	No	No	No	No	No	No	No	Yes	No
Maternal FE	No	No	No	No	No	No	No	Yes	No	No	No	No	No	No	No	Yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors (in parentheses) are clustered at the neighborhood level for models with neighborhood fixed effects, at the postcode level for models with postcode fixed effects, at the street level for models with street fixed effects, at the hospital level for models with hospital fixed effects, and at the mother level for models with mother fixed effects.

Note: The analysis includes mothers observed during the period from 2011 to 2017. *Birthweight (BW)* is reported in grams. *Low BW*, *Very low BW* and *Extremely low BW* are dummy variables indicating newborns weighing up to 2,500, 2,000, and 1,500 grams, respectively. The explanatory variable *Victim (Pregnancy)* indicates whether the mother was a victim of robbery or theft during pregnancy. Controls for the specifications with neighborhood, postcode, street, and hospital fixed effects include dummies for the mother's age, race, marital status, whether the mother has ever received a cash transfer from the government, level of education, occupation, and dummies for the number of children born alive and stillbirths from previous pregnancies. For the specifications with maternal fixed effects, controls include dummies for the mother's age, race, marital status, education, and occupation, as well as dummies for the number of stillbirths from previous pregnancies, gestation order, and birth interval (time between conceptions). All regressions include month-of-conception fixed effects.

Table 2: Effect of crime victimization on additional outcomes

	Gestatation (days)	*Gestation (<259 days)*	*Gestation (<224 days)*	*Gestation (<196 days)*	*1st minute APGAR*	*5th minute APGAR*
	(1)	(2)	(3)	(4)	(5)	(6)
Victim	−0.2090	0.0004	0.0027	0.0006	−0.0112	−0.0008
(Pregnancy)	(0.1373)	(0.0027)	(0.0012)**	(0.0006)	(0.0111)	(0.0071)
Mean dep. var.	269.5334	0.1041	0.0141	0.0045	8.4117	9.3751
Clusters	32,405	32,405	32,405	32,405	31,982	31,985
Observations	1,433,140	1,433,140	1,433,140	1,433,140	1,390,589	1,391,334

	Emergency C-section	*Prenatal visits*	*Female*	*Early neonatal (1 week)*	*Neonatal (4 weeks)*	*Infant (1 year)*
	(1)	(2)	(3)	(4)	(5)	(6)
Victim	0.0030	−0.0889	0.0042	0.0000	0.0002	0.0000
(Pregnancy)	(0.0033)	(0.0229)***	(0.0045)	(0.0005)	(0.0006)	(0.0006)
Mean dep. var.	0.2006	8.0571	0.4882	0.0032	0.0041	0.0055
Clusters	32,405	32,246	32,402	32,405	32,405	32,405
Observations	1,433,140	1,417,513	1,432,927	1,433,140	1,433,140	1,433,140

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the neighborhood level in parentheses.

Note: The analysis includes mothers observed during the period from 2011 to 2017. *Birthweight (BW)* is reported in grams. *Low BW*, *Very low BW* and *Extremely low BW* are dummy variables indicating newborns weighing up to 2,500, 2,000, and 1,500 grams, respectively. Explanatory variable *Victim* indicate whether the mother was a victim of robbery or theft during pregnancy. Control variables include dummies for the mother's age, race, marital status, education, occupation, whether she has ever received a government cash transfer, and the number of children born alive and stillbirths from previous pregnancies. All regressions include neighborhood and month of conception fixed effects.

Table 3: Effect of crime victimization on hospitalization

	Hospitalization (3 years)	*Hospitalization (1st year)*	*Hospitalization (28 days)*	*Hospitalization (7 days)*	*Cost*	*ICU (Neonatal)*	*ICU*	*Length of Stay*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Victim	0.0061	0.0060	0.0045	0.0041	0.0782	0.0045	0.0040	0.1451
(Pregnancy)	(0.0034)*	(0.0032)*	(0.0028)	(0.0027)	(0.0389)**	(0.0015)***	(0.0016)**	(0.0851)*
Mean dep. var.	0.1207	0.0986	0.0665	0.0598	361.5608	0.0188	0.0226	1.1979
Clusters	26,077	26,077	26,077	26,077	26,077	26,077	26,077	26,077
Observations	942,736	942,736	942,736	942,736	942,736	942,736	942,736	942,736

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the neighborhood level in parentheses.

Note: The analysis includes mothers observed during the period from 2011 to 2017. *Hospitalization (3 years)* is a dummy variable indicating whether the infant was hospitalized at any point during the first three years of life. *Hospitalization (1st year)*, *Hospitalization (28 days)*, and *Hospitalization (7 days)* are similar indicators for hospitalizations occurring within the first year, first 28 days, and first seven days of life, respectively. *Cost* refers to the logarithm of the total cost of hospitalization. *Intensive Care Unit (Neonatal)* and *Intensive Care Unit* are dummy variables indicating whether the infant was admitted to a neonatal or a regular intensive care unit. We replace zero values with a value of 0.01 before taking logs. *Length of Stay* measures the total number of days the infant spent in the hospital. Explanatory variable *Victim (Pregnancy)* indicates whether the mother was a victim of robbery or theft during pregnancy. Control variables include dummies for the mother's age, race, marital status, education, occupation, whether she has ever received a government cash transfer, and the number of children born alive and stillbirths from previous pregnancies. All regressions include neighborhood and month of conception fixed effects.

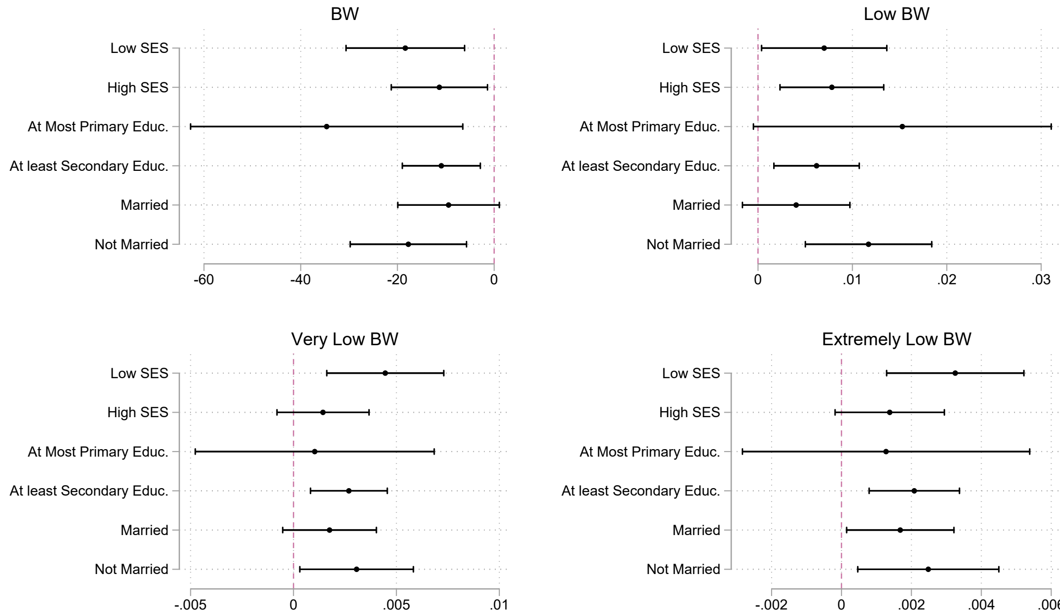

Figure 1: Effect of Victimization on Birthweight by Socioeconomic Status

Note: The individual graphs depict estimates on BW and low BW classifications by proxies of socio-economic status. Low SES is an indicator taking a value of 1 if a mother has been a recipient of Bolsa Familia, the Brazilian cash transfer programme. Information on Bolsa Familia is available by linking information from CadUnico to birth records. *At most primary education* denotes an indictor for females who have at time of birth (of their last child conceived in the data) at most completed primary education. Married is an indicator for being in a stable relationship (either married or in stable relationship). Information on maternal education and marital status is available from birth records. The coefficients displayed come from estimates using our preferred specification (column (2) and (10) of Table 1) with error bars using 95 percent confidence intervals. Control variables include dummies for the mother's age, race, marital status, education, occupation, whether she has ever received a government cash transfer, and the number of children born alive and stillbirths from previous pregnancies, less the variables which we use to split the sample for each pairing. All regressions include neighborhood and month of conception fixed effects and the usual set of controls.

Appendices to accompany *The Intergenerational Costs of Crime: Evidence from Maternal Victimization in Brazil*
Lívia Menezes and Martin Foureaux Koppensteiner

Appendix A Additional Tables and Figures

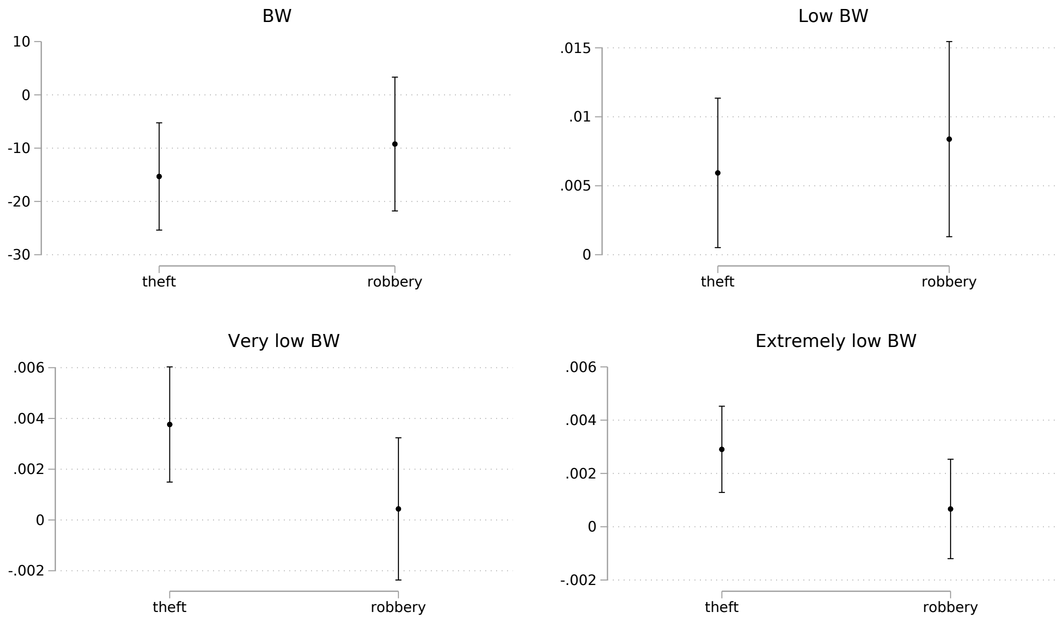

Figure A1: Effect of Victimization by Type of Crime

Note: The coefficients displayed come from estimates using our preferred specification (column (2) and (10) of Table 1) estimated separately for victimization of theft and robbery. The analysis includes mothers observed during the period from 2011 to 2017. *Birthweight (BW)* is reported in grams. *Low BW*, *Very low BW* and *Extremely low BW* are dummy variables indicating newborns weighing up to 2,500, 2,000, and 1,500 grams, respectively. Control variables include dummies for the mother's age, race, marital status, education, occupation, whether she has ever received a government cash transfer, and the number of children born alive and stillbirths from previous pregnancies. All regressions include neighborhood and month of conception fixed effects.

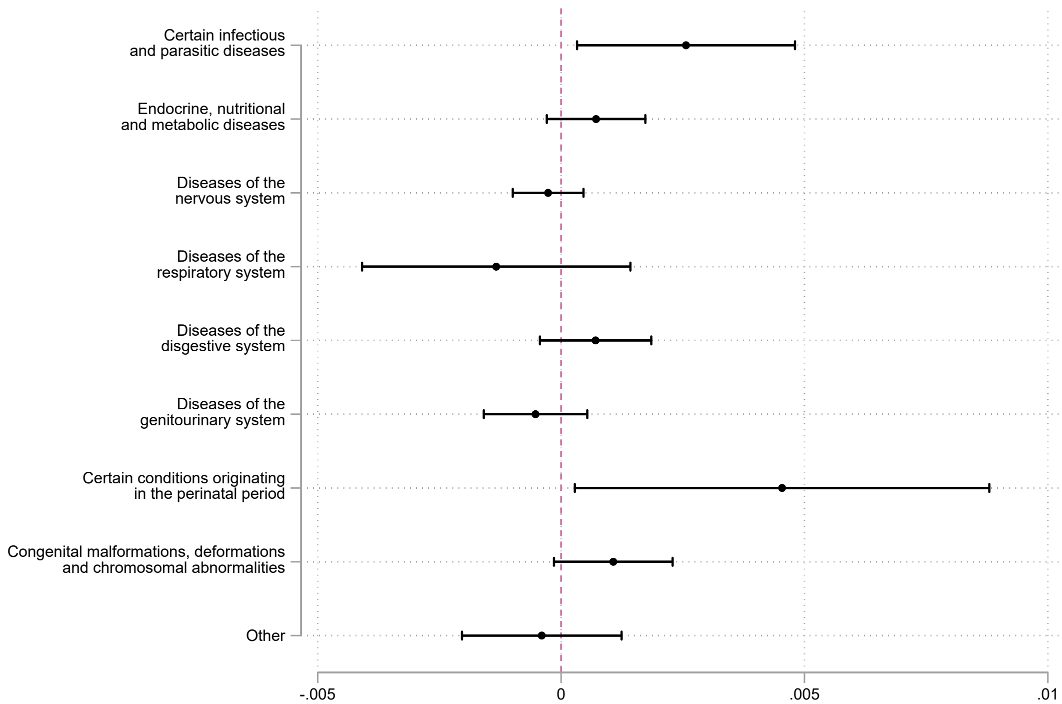

Figure A2: Effect of Victimization on Hospitalization by Cause

Note: Classification by cause of hospitalization is based on WHO ICD-10 using starting letters to group causes of hospitalization. We focus on the most common groups of causes and summarize the remainder in *Other*. The coefficients displayed come from estimates using our preferred specification (column (2) and (10) of Table 1) with error bars using 95 percent confidence intervals. The analysis is based on the sample of children hospitalized in the first year after birth. Control variables include dummies for the mother's age, race, marital status, education, occupation, whether she has ever received a government cash transfer, and the number of children born alive and stillbirths from previous pregnancies. All regressions include neighborhood and month of conception fixed effects.

Table A1: Effect of crime victimization on birthweight - robustness checks

Panel A - Baseline: Excluding related with perpetrator victims

	BW	*Low BW*	*Very Low BW*	*Extremely Low BW*
	(1)	(2)	(3)	(4)
Victim	−13.0352	0.0069	0.0025	0.0021
(Pregnancy)	(4.6011)***	(0.0026)***	(0.0011)**	(0.0007)***
Mean dep. var.	3,159.0424	0.0804	0.0110	0.0045
Clusters	32,405	32,405	32,405	32,405
Observations	1,433,140	1,433,140	1,433,140	1,433,140

Panel B - All victims included

	BW	*Low BW*	*Very Low BW*	*Extremely Low BW*
	(1)	(2)	(3)	(4)
Victim	−17.3768	0.0082	0.0025	0.0019
(Pregnancy)	(4.3830)***	(0.0025)***	(0.0010)**	(0.0007)***
Mean dep. var.	3,158.9928	0.0804	0.0110	0.0045
Clusters	32,425	32,425	32,425	32,425
Observations	1,434,470	1,434,470	1,434,470	1,434,470

Panel C - Excluding injured victims

	BW	*Low BW*	*Very Low BW*	*Extremely Low BW*
	(1)	(2)	(3)	(4)
Victim	−17.0973	0.0083	0.0020	0.0028
(Pregnancy)	(4.4450)***	(0.0025)***	(0.0007)***	(0.0010)***
Mean dep. var.	3,159.0009	0.0804	0.0045	0.0110
Clusters	32,421	32,421	32,421	32,421
Observations	1,434,179	1,434,179	1,434,179	1,434,179

Panel D - Excluding injured or related with perpetrator victims

	BW	*Low BW*	*Very Low BW*	*Extremely Low BW*
	(1)	(2)	(3)	(4)
Victim	−12.6790	0.0070	0.0027	0.0021
(Pregnancy)	(4.6747)***	(0.0027)***	(0.0011)**	(0.0007)***
Mean dep. var.	3,159.0502	0.0804	0.0110	0.0045
Clusters	32,403	32,403	32,403	32,403
Observations	1,432,865	1,432,865	1,432,865	1,432,865

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the neighborhood level in parentheses.

Note: The analysis includes mothers observed during the period from 2011 to 2017. *Panel A* presents our baseline specification for comparison (Table 1, columns 2 and 10). This sample excludes all victimization cases in which we identified any relationship between the victim and the perpetrator. *Panel B* includes all victims, that is, it combines the sample from *Panel A* with the cases where a relationship between the victim and perpetrator was identified, potentially capturing instances of domestic disturbances/abuse. *Panel C* excludes from the baseline sample all cases in which the victim was injured, that is, it uses the sample from *Panel A* but removes injured victims. *Panel D* further restricts the baseline sample by excluding both injured victims and cases involving a known relationship between victim and perpetrator, effectively removing potential domestic violence cases as well. *Birthweight (BW)* is reported in grams. *Low BW*, *Very low BW* and *Extremely low BW* are dummy variables indicating newborns weighing up to 2,500, 2,000, and 1,500 grams, respectively. Explanatory variable *Victim (Pregnancy)* indicates whether the mother was a victim of robbery or theft during pregnancy. Control variables include dummies for the mother's age, race, marital status, education, occupation, whether she has ever received a government cash transfer, and the number of children born alive and stillbirths from previous pregnancies. All regressions include neighborhood and month of conception fixed effects.

Table A2: Effect of crime victimization on birthweight - including women victimized more than once during pregnancy

	BW	*Low BW*	*Very low BW*	*Extremely low BW*
	(1)	(2)	(3)	(4)
Victim	−12.1859	0.0069	0.0021	0.0019
(Pregnancy)	(4.5267)***	(0.0026)***	(0.0010)**	(0.0007)***
Mean dep. var.	3,159.0468	0.0804	0.0110	0.0045
Clusters	32,406	32,406	32,406	32,406
Observations	1,433,452	1,433,452	1,433,452	1,433,452

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the neighborhood level in parentheses.

Note: The analysis includes mothers observed during the period from 2011 to 2017. Unlike our baseline specification, in this case the explanatory variable *Victim* also includes women who were victimized more than once during pregnancy. *Birthweight (BW)* is reported in grams. *Low BW*, *Very low BW* and *Extremely low BW* are dummy variables indicating newborns weighing up to 2,500, 2,000, and 1,500 grams, respectively. Explanatory variable *Victim* indicate whether the mother was a victim of robbery or theft during pregnancy. Control variables include dummies for the mother's age, race, marital status, education, occupation, whether she has ever received a government cash transfer, and the number of children born alive and stillbirths from previous pregnancies. All regressions include neighborhood and month of conception fixed effects.

Table A3: Balance Test - Full Sample

Variable	Mean Victimized	SD Victimized	Mean Non-victimized	SD Non-victimized	Norm. Diff
Age	27.6710	6.2293	26.8438	6.6113	0.1288
20 or less	0.1458	0.3529	0.2031	0.4023	-0.1514
21 to 35	0.7368	0.4404	0.6881	0.4633	0.1077
35 and beyond	0.1174	0.3219	0.1087	0.3113	0.0275
White	0.3824	0.4860	0.3594	0.4798	0.0476
Black	0.0685	0.2526	0.0821	0.2746	-0.0515
Asian	0.0068	0.0823	0.0064	0.0798	0.0049
Mixed	0.5412	0.4983	0.5500	0.4975	-0.0177
Indigenous	0.0010	0.0322	0.0020	0.0452	-0.0255
Single	0.4375	0.4961	0.3956	0.4890	0.0851
Married	0.4316	0.4953	0.4442	0.4969	-0.0254
Widowed	0.0030	0.0543	0.0026	0.0506	0.0076
Separated/ divorced	0.0232	0.1505	0.0159	0.1250	0.0528
Stable union	0.1048	0.3063	0.1418	0.3488	-0.1127
No education	0.0009	0.0302	0.0029	0.0538	-0.0458
1 to 3 yrs. educ.	0.0083	0.0906	0.0246	0.1549	-0.1285
4 to 7 yrs. educ.	0.0954	0.2938	0.1780	0.3825	-0.2422
8 to 11 yrs. educ.	0.6052	0.4888	0.6029	0.4893	0.0047
12 or more yrs. educ.	0.2902	0.4539	0.1916	0.3935	0.2321

Notes: This table reports the means and standard deviations of mother characteristics by victimization status, as well as the normalized difference between means. Normalized differences are defined as $\Delta = (\bar{X}_1 - \bar{X}_0)/\sqrt{S_1^2 + S_0^2}$.

Table A4: Balance Test - Alternative Sample

Variable	Mean Victimized	SD Victimized	Mean Non-victimized	SD Non-victimized	Norm. Diff
Age	27.6710	6.2293	27.1609	6.3204	0.0813
20 or less	0.1458	0.3529	0.1700	0.3756	-0.0664
21 to 35	0.7368	0.4404	0.7242	0.4469	0.0284
35 and beyond	0.1174	0.3219	0.1058	0.3076	0.0368
White	0.3824	0.4860	0.3760	0.4844	0.0132
Black	0.0685	0.2526	0.0767	0.2662	-0.0316
Asian	0.0068	0.0823	0.0079	0.0886	-0.0129
Mixed	0.5412	0.4983	0.5383	0.4985	0.0058
Indigenous	0.0010	0.0322	0.0012	0.0339	-0.0060
Single	0.4375	0.4961	0.4519	0.4977	-0.0290
Married	0.4316	0.4953	0.4059	0.4911	0.0521
Widowed	0.0030	0.0543	0.0027	0.0523	0.0056
Separated/ divorced	0.0232	0.1505	0.0243	0.1538	-0.0072
Stable union	0.1048	0.3063	0.1152	0.3193	-0.0332
No education	0.0009	0.0302	0.0009	0.0306	0.0000
1 to 3 yrs. educ.	0.0083	0.0906	0.0113	0.1058	-0.0305
4 to 7 yrs. educ.	0.0954	0.2938	0.1205	0.3256	-0.0809
8 to 11 yrs. educ.	0.6052	0.4888	0.6030	0.4893	0.0045
12 or more yrs. educ.	0.2902	0.4539	0.2642	0.4409	0.0581

Notes: This table reports the means and standard deviations of mother characteristics by victimization status, as well as the normalized difference between means. Normalized differences are defined as $\Delta = (\bar{X}_1 - \bar{X}_0)/\sqrt{S_1^2 + S_0^2}$.

Table A5: Balance Test - Maternal FE Sample

Variable	Mean Victimized	SD Victimized	Mean Non-victimized	SD Non-victimized	Norm. Diff
Age	26.2311	5.8105	25.1317	6.0254	0.1857
20 or less	0.1810	0.3851	0.2598	0.4385	-0.1910
21 to 35	0.7465	0.4351	0.6816	0.4659	0.1440
35 and beyond	0.0724	0.2592	0.0587	0.2350	0.0554
White	0.3702	0.4830	0.3372	0.4728	0.0690
Black	0.0768	0.2663	0.0925	0.2897	-0.0564
Asian	0.0042	0.0650	0.0067	0.0817	-0.0339
Mixed	0.5469	0.4979	0.5613	0.4962	-0.0290
Indigenous	0.0019	0.0434	0.0023	0.0475	-0.0088
Single	0.4743	0.4994	0.4337	0.4956	0.0816
Married	0.4070	0.4914	0.4084	0.4915	-0.0028
Widowed	0.0018	0.0425	0.0017	0.0409	0.0024
Separated/ divorced	0.0126	0.1118	0.0106	0.1026	0.0186
Stable union	0.1043	0.3058	0.1456	0.3527	-0.1251
No education	0.0005	0.0213	0.0015	0.0383	-0.0323
1 to 3 yrs. educ.	0.0086	0.0923	0.0157	0.1244	-0.0648
4 to 7 yrs. educ.	0.1129	0.3166	0.1996	0.3997	-0.2405
8 to 11 yrs. educ.	0.6161	0.4864	0.6009	0.4897	0.0311
12 or more yrs. educ.	0.2620	0.4398	0.1824	0.3862	0.1923

Notes: This table reports the means and standard deviations of mother characteristics by victimization status, as well as the normalized difference between means. Normalized differences are defined as $\Delta = (\bar{X}_1 - \bar{X}_0)/\sqrt{S_1^2 + S_0^2}$.

Appendix B: Balance Tests and Sample Comparability

This appendix provides evidence on the comparability of treated and control observations across the different samples used in the analysis. As discussed in Section 3, identification relies on the assumption that, conditional on observed characteristics and fixed effects, exposure to victimization during pregnancy is orthogonal to unobserved determinants of birth outcomes. We assess this assumption by examining balance in observable characteristics using normalized differences rather than t-tests, as the large sample sizes render statistical significance uninformative.

Normalized differences are defined as

$$\Delta = \frac{\bar{X}_1 - \bar{X}_0}{\sqrt{S_1^2 + S_0^2}}, \tag{2}$$

where $\bar{X}_1$ and $\bar{X}_0$ denote the means of the treated and control groups, respectively, and S_1^2 and S_0^2 their corresponding variances. This measure is invariant to sample size and provides a scale-free metric of imbalance. As a benchmark, absolute values below 0.20 are typically interpreted as indicating good balance.

B.1 Main Sample

Appendix Table A2 reports balance statistics for the full sample, comparing mothers who were victimized during pregnancy to those who were not. Overall, normalized differences are small for the majority of demographic, socioeconomic, and pregnancy-related characteristics. Most absolute normalized differences are well below 0.20, including for maternal age, race, marital status, education, and reproductive history.

Some characteristics display modest differences, particularly age and marital status. However, the magnitudes of these differences remain limited, and all corresponding variables are explicitly included as controls in the regression specifications, together with neighborhood and month-of-conception fixed effects. Consistent with the identifying assumption described in Section 3, these results indicate that, conditional on observables and fixed effects, treated and control mothers in the full sample are drawn from broadly comparable populations.

B.2 Alternative Sample: Ever-Victimized Mothers

Appendix Table A3 presents balance statistics for the alternative sample that restricts the control group to mothers who are eventually victimized, comparing those exposed during pregnancy to those victimized only after childbirth. This specification underlies the timing-based identification strategy discussed in Section 3, which assumes that, conditional on observables and fixed effects, the timing of victimization relative to pregnancy is orthogonal to unobserved determinants of birth outcomes.

Consistent with this design, normalized differences in this sample are uniformly small across all reported characteristics and are generally smaller in magnitude than in the full sample. Nearly all absolute normalized differences are close to zero and well below conventional thresholds. This enhanced balance provides direct support for the plausibility of the timing-based identifying assumption and suggests that treated and control mothers in this sample differ primarily in the timing of victimization rather than in underlying characteristics.

B.3 Maternal Fixed Effects Sample

Appendix Table A4 reports balance statistics for the maternal fixed effects sample, which exploits within-mother variation across multiple pregnancies. In this specification, identification relies exclusively on variation in exposure to victimization across pregnancies to the same mother, thereby differencing out all time-invariant maternal characteristics, including baseline health, long-run socioeconomic status, preferences, and reporting behavior.

Normalized differences in this sample mainly reflect predictable life-cycle changes across pregnancies, such as maternal age progression, parity, and birth spacing. These time-varying factors are explicitly controlled for in the fixed effects specifications. As a result, any remaining differences in observable characteristics do not threaten identification, provided that time-varying confounders are adequately accounted for, as described in Section 3.

B.4 Summary

Taken together, balance tests based on normalized differences provide reassuring evidence in support of the identifying assumptions underlying the empirical strategy. In the full sample, normalized differences are generally small and are addressed through rich covariate controls and fixed effects. The alternative ever-victimized sample exhibits particularly strong balance, reinforcing the plausibility of the timing-based identification strategy. Finally, the maternal fixed effects design eliminates all time-invariant heterogeneity by construction, with remaining normalized differences reflecting mechanical life-cycle dynamics rather than selection into victimization.

Appendix B Birth data and descriptive statistics

Appendix Table A1 reports descriptive statistics for birth outcomes and maternal characteristics, separately for mothers exposed to victimization during pregnancy (treated) and for those not exposed (control). The figures are broadly similar across groups, which reassures us that treatment and control samples are comparable once rich covariates and fixed effects are included in the analysis.

B.1 Birth data and descriptive statistics

Our main measure of newborn health is birthweight (BW). As shown in Appendix Table A1, the mean BW in our sample is around 3,150 grams.[22] To study the lower tail of the distribution, we construct indicator variables for low BW (below 2,500 grams). The incidence of low BW is 8.7 percent,[23] and mean fetal growth – measured as BW divided by gestational weeks – is 81 grams per week.

We also make use of information on Apgar scores, which assess the newborns condition based on activity (muscle tone), pulse (heart rate), grimace (reflex irritability), appearance (skin color), and respiration (breathing effort).[24] Scores are reported at one and five minutes after birth. The mean Apgar scores in our sample are 8.4 and 9.4, respectively.

The mean gestational length is 272 days. Preterm delivery (less than 37 weeks) occurs in 12.7 percent of births, while very preterm delivery (less than 34 weeks) accounts for 1.8 percent.[25]

Multiple births are relatively rare: 2.1 percent of pregnancies result in twins and only 0.1 percent in triplets or higher-order multiples. The remaining 97.6 percent are singleton

[22] This is about 300 grams lower than the mean BW in the United States (Donahue et al., 2010).

[23] This rate is comparable to the U.S., where the incidence was 8.2 percent in 2016 (Martin et al., 2016).

[24] Each category is scored from 0 to 2. Overall scores of 7 and above are considered normal, and a score of 10 indicates optimal health.

[25] These figures imply a higher preterm rate in Brazil compared to the U.S. (9.9 percent in 2016), but a somewhat lower rate of very preterm births (2.8 percent in the U.S.) (Martin et al., 2016).

births. Cesarean delivery is very common, occurring in nearly 60 percent of births. Only about 20 percent of these are classified as emergency procedures, i.e. initiated after the onset of labor.[26]

Appendix Table A1 also reports maternal characteristics. The mean maternal age is 27 years, with 38.3 percent of mothers younger than 25. Regarding race, 49 percent self-identify as mixed race, 35.2 percent as white, and 6.8 percent as Black. Marital status is also recorded: 34.5 percent of mothers are single, 47 percent married, 15.6 percent in a stable union, 0.3 percent widowed, and 1.5 percent separated. Educational attainment is relatively low: 20.7 percent of mothers have up to 7 years of schooling, 58 percent completed 811 years, and 19 percent have 12 or more years of schooling.

Appendix C Probabilistic Matching of Hospitalization Records

Hospitalization records (SIH) do not contain a unique identifier that can be directly linked to birth records. We therefore employ a probabilistic matching procedure based on child's date of birth and postcode of residence. Specifically, we match each hospitalization record to the corresponding birth record if both variables coincide uniquely. In cases with multiple potential matches (e.g., twins, or multiple births on the same day sharing a postcode), we apply additional filters using maternal age (in years) and sex of the child. When duplication persists after applying the filter, we drop the ambiguous records. This approach minimizes false positives but may lead to under-matching. As a result, the hospitalization linkage has a lower match rate than the other datasets, which should be interpreted as introducing noise (attenuation) rather than bias into the estimates.

[26] High rates of planned cesarean delivery in Brazil have been widely documented (Barros et al., 1991).